SCIENCE.

FRIDAY, AUGUST 14, 1885.

OFFICIAL SCRUTINY OF THE U. S. COAST-SURVEY.

SERIOUS charges have been brought by the authorities in Washington against the administration of the U. S. coast-survey. During many days past, the newspapers, under more or less sensational headings, have given currency to statements and insinuations of a damaging character. Some of the articles which have come under our eye are obviously wrong, some contain half-truths, and some are inspired with animosity toward particular men, or toward the prosecution of purely scientific work. Up to the time when these lines are written, we have seen nothing in the journals which bears the stamp of official accuracy; but there appeared on Friday last what purports to be a summary of the results attained by a committee of investigation appointed by the treasury department. If so much is to be said in public, as is contained in this statement, we regret that the charges are not authenticated by a signature, and that the circumstances under which the inquiry has been conducted are not clearly made known. The credit of an important branch of the public service, as well as the personal standing of its leading officers, is involved in these criminations. A decent regard for justice, and a fair consideration for those who have long maintained a good report, require something more than anonymous and semi-official communications, sent out by telegraph, in which it is not possible to discriminate the pen of the reporter from that of the authorized investigator, and in which it is still harder to determine what are the charges of the complainants, and what are the conclusions of the tribunal. Either reticence should govern an investigation until some conclusion is reached which can be openly made known, or else there should be sufficient publicity in the conduct of the inquiry to acquaint the public with the extenuating circumstances of the defence, and the answers which are made to the preferred complaints. Unless we are misled by these unofficial statements, it appears that the superintendent of the survey has been deposed from his office for alleged mismanagement, that the assistant in charge at Washington has been first removed and then restored, and that several persons employed by the survey (chiefly in subordinate relations to the service) have been brought under censure.

We have no disposition to apologize for, or to screen, continued neglect of duty, or abuse of the high responsibilities which are attached to the conduct of an important post; but certain facts of an explanatory character should certainly accompany expressions of blame. The friends of the superintendent are aware that he has been for a long while a sufferer from a painful disease. He has been advised, as we are informed, to seek relief from acute and distressing attacks by the use of agents which are extra hazardous. There is no doubt that his efficiency as an administrative officer has thus been seriously impaired. It would have been well, under circumstances of so much responsibility, if he could have been quietly retired in view of the long-continued, efficient, and able services which he has rendered to the country. Failure resulting from physical infirmity, and from the employment, however injudicious, of the prescribed means of relief, is failure still, so far as the conduct of a public office is concerned; but it is not the failure of a dissipated man indulging in vice. The accused is not to be condemned like one who has surrounded himself with bad associates, or who has appropriated to his personal gratification the public money. His previous record of fidelity, application, and uprightness should be remembered. A sharp distinction should be drawn between erroneous methods of control which

he has initiated, and those bad traditional forms which are likely to grow up in any public office unless the most vigilant watch is kept. If the record of the superintendent is good, — and we believe that it is, up to the time of a great domestic sorrow, and the subsequent incursions of disease, — the judgment of the government and of the public should be very different from that which would be due to a dishonest, incapable, or dissipated man. We sincerely believe, that, when all the facts are brought out, our judgment will prove to be correct, and that the bad administration attributed to the superintendent will not be without extenuating considerations. The honors which have been won by the coast-survey abroad and at home, for thoroughness, under arduous and complicated circumstances, are honors which have been won by those who have administered its affairs.

While awaiting further information, there are some known facts, and some known principles, which it is worth while to bear in mind, particularly as there is always a multitude ready to raise a hue and cry if any determined opponent leads off in throwing out suspicions.

Without the slightest disposition to screen official mismanagement, if it has been discovered, we must caution our readers against giving credence to insinuations and rumors. All who are under implied censure have a right to be fully heard, and to bring all the facts which are explanatory of their conduct to the eye of a qualified tribunal. They have a right to protest against the arbitrary exercise of personal authority, or against the judicial methods of a star-chamber or a drum-head court-martial. No political purpose, no personal dislike, no disbelief in science, should be allowed, unquestioned, to throw discredit upon a branch of the public service, or dishonor upon a corps hitherto regarded as exemplary in all its official work.

The work of the coast-survey, during its long history, has been of the highest character. For nearly seventy years it has been approved by successive congresses and administrations, and by navigators, merchants, and men of exact science. It has received the highest encomiums of foreigners who were qualified to judge of its merits, and were interested in pointing out its defects. The five superintendents — Hassler, Bache, Benjamin Pierce, Patterson, and Hilgard — have each, in different ways, improved its methods, and upheld its efficiency. The officers just displaced have grown up in the service, and have won promotion by the ability and fidelity with which they have discharged their great responsibilities. The presumptions of official rectitude are in their favor until positive faults are pointed out. They are entitled by the principles of good government, as well as by their individual services, to all the opportunities they may desire for explanation or defence; and any premature opinion is unfair, especially if it is affected by personal prejudices, or is based upon a lack of appreciation for scientific researches.

In the conduct of such a bureau as the coast-survey, a large amount of discretion must be left to the chief. He, and he only, can determine a vast number of questions which pertain to the selection of assistants for different kinds of work, the choice of fields of labor, the discrimination between services which have an obvious relation to some immediate want of the public, and those which may be just as serviceable, but are recondite, and unintelligible to the uninformed. It is impossible to mark out the duties of the highest assistants by such rules as may be applied to the clerical services of an ordinary counting-room. In order that the results of the survey may be accurate and trustworthy, — the only results which are worth having, — costly instruments must be bought and used, and must afterwards be thrown aside, because other instruments are better, or because their work is done. Still larger outlays are requisite, in order that elaborate and important fundamental inquiries may be prosecuted by men who are trained to exact scientific methods. A staff of learned and experienced investigators is absolutely essential to the conduct of such a national undertaking as the coast-survey.

Nevertheless, all this scientific research is appreciated by a very small number of persons. Indeed, the more valuable it is, the less obvious may be its merits. Every seaman knows the value of a good chart: not every seaman, not every scholar, not every statesman, knows the conditions by which a good chart is produced. It is only the expert who appreciates the subtle sources of error which must be eliminated: he only knows the infinitude of mathematical, physical, astronomical, and geodetic problems, which are involved in an endeavor to portray faithfully such a coast line as that of the United States, and to keep the portrayal in accurate correspondence with the changing sands.

The judge of what to do, and how to do it, must be the superintendent. Congress must say how much money may be spent, and the secretary of the treasury must exercise an authority over the methods of expenditure: but the master of the works must be the head of the survey; and, although he is liable to error, like the general in the field, or the seaman on the deep, the ultimate results, attained under his guidance, are the criterion of his scientific efficiency.

In the zeal for civil-service reform, which has characterized the new administration, it will not be surprising if outlays for scientific observation, experiment, and research should be regarded as questionable if not extravagant. It is not to be wondered at, that an auditor of accounts should consider as needless, expenditures which experience has shown to be absolutely necessary for the efficient management of a scientific bureau. It will not be strange if a commission of government officials pronounces many of the investigations of the coast-survey to be incomplete, useless, or unduly costly. It will be easy to gain a reputation for economy, and for discovering the faults of preceding administrations, by striking at work, the methods of which, from their very nature, are incomprehensible to the public. It is easy to furnish witticisms to innumerable writers by a judicious repetition of scientific technicalities. But, happily, Congress is not likely to be misled by such combined misapprehensions and misrepresentations. The president, unless we mis-read his official and personal character, will insist upon wise economy. Beyond the administration and Congress, there lies an appeal to the intelligence of the people, who certainly do not want parsimony in the study of the sea-coast. Honesty and accountability will be demanded by the public in all branches of the government service: they will rejoice in every check which may be devised to prevent the misappropriation of funds, but they will not want the efficiency of the coast-survey impaired. An administration will indeed appear awry, which proposes in one breath to restore the navy to efficiency, and in another to interfere with the accurate study of the coast, and with the perfection of our knowledge of harbors and reefs. Let there be fair play in considering the affairs of the coast-survey, and we shall have no fears of the result.

LETTERS TO THE EDITOR.

A mad stone.

THE Sedalia and other papers lately contained accounts of the application of a 'mad stone' to a Mr. Girard of this city, who had been bitten by a supposed mad dog.

The stone was owned by Mr. J. M. Dickson of Kansas City, who advertises the use of the stone, and states that it has been in possession of his family for more than a hundred years, and was brought by one of the family from Scotland. From the large number of references given in Mr. Dickson's advertisement to the mayor and other officials, and physicians of Kansas City, we may take it as true that Mr. Dickson is honest in his belief as to the virtues and history of the stone.

To a reporter Mr. Dickson made a statement that he had applied the stone to more than five hundred cases of bites by various kinds of mad animals and wild skunks; his opinion evidently being, that the bite of this animal, whether rabid or not, will produce hydrophobia. He gave the method of application, which was to place the stone upon the wound, or upon an abrasion of the skin made on any part of the body, first soaking the stone in sweet milk. He stated, that, if the person contained any virus, the stone would adhere to the wound or abrasion until it was saturated with the poison, when it would fall off; and that it was then cleaned by again soaking it in sweet milk, and this was repeated until the stone would no longer adhere.

We may presume, that, of the five hundred treated by him, a large number had been bitten by animals which were not mad; and statistics show, that, of those bitten by dogs which are mad, not more than one-third to one-half will have hydrophobia; and yet we can hardly suppose, that, of five hundred persons who believed themselves to be in danger of hydrophobia, not one would have taken it even if no preventive measures had been taken. Mr. Dickson

states, that no case treated by him has developed into hydrophobia.

At the time of Mr. Dickson's visit to Sedalia, I had the opportunity of seeing the stone for a few minutes, and found it to be a fossil coral of the genus Favosites. It was of rather small size, only about three-fourths of an inch across, and was of hemispherical shape, with one side cut so as to present a smooth surface. The fossil seemed to be silicified, a part of the tubes being filled almost to the ends, and a part open. The tube cavities on the flat surface generally presented open spaces between the diaphragms or tabulae, making the stone more or less cellular or porous. From the slight examination I made of the stone, I judge it to be Favosites gothlandica Lam., if from Scotland; and, if it is American, F. hemisphericus Y. and S.

I have since seen Mr. Girard; and I learn from him, and also from the Sedalia agent of the Adams express company, that the stone was first soaked in sweet milk without having any effect upon the color of the milk. It was then applied to the arm, and adhered so tightly, that, on turning the arm over and shaking it, the stone still clung to it. About three times the stone was taken from the arm and soaked in milk, and it then turned the milk a greenish color. At last the stone would no longer adhere to the wound, and the cure was pronounced complete.

Has any competent person made proper tests of reputed mad stones? Are these persons mistaken about the stone adhering tightly? Would any similar porous stone adhere the same way? Are the persons also mistaken about the change in the color of the milk? In short, will any stone have any effect on virus in a person's blood? F. A. SAMPSON.

Sedalia, Mo., July, 1885.

[We may add as a final query, How did such a superstition arise? — ED.]

The inscription rocks on the island of Monhegan.

During a recent visit to the island of Monhegan, Me., my attention was called by Mr. P. C. Manning of Portland to the so-called inscriptions described and figured by Schoolcraft in his 'Indian tribes,' vol. vi. p. 610. The inscriptions are on a small island, Menana, which is separated from Monhegan island proper by a narrow channel. The principal inscription, that figured by Schoolcraft, is on the nearly vertical face of a small cliff about five feet high, situated a few rods north and east from the fog-signal station. The country rock of both islands is a black or dark-gray rock different from any rock I have seen in Maine except at one other locality. A lithological description of this rock is reserved for the present. It shows great numbers of veins. Part of these veins are of white granite, or sometimes of white quartz; but many are black, like the surrounding rock, and differ from it simply in fineness of grain. When weathered, even the blackest of the rocks become dark gray in color. As the various layers differ so much in granular condition, and somewhat in composition, they naturally weather and fracture very differently. Some of the rock is quite massive, with no regular fracture: other layers fracture quite prismatically, almost like slates. The rocks are everywhere weathered into forms unusually varied, and often fantastic; the veins sometimes weathering faster than the contiguous rock into furrows, at other times into ridges. The joints and veins are often arranged systematically.

When one first sees the inscription rock, he cannot fail to notice that the appearance is as if a tablet had been prepared upon the surface of the rock, not horizontally, but obliquely. There are two parallel furrows about one-half an inch deep, and eight inches apart; and the so-called letters are on this 'tablet.' The tablet has a fine-pitted surface of weathering quite even and flat. The surrounding rock is more coarsely pitted. Examination shows that this apparent tablet is simply the exposed edge of a fine-grained vein which penetrates the coarser-grained rock obliquely. This vein shows both on top of the rock and also on the side. The parallel furrows which enclose the so-called inscription tablet are simply furrows of weathering at the sides of the vein. The supposed letters are composed of straight furrows intersecting each other obliquely, so that most of them are some modification of the letters V and X. A cross-section of these furrows ends in a sharp angle enclosed between curved lines, like the sinus of a crenate leaf. At the base of the furrows I invariably found a crack in the rock, though sometimes not readily without the aid of a magnifier. There are two systems of these joints, — one nearly vertical, the other nearly at right angles to the sides of the vein. Nearly all the furrows forming the supposed inscription belong to these two systems of joints: a few are aberrant, and two are horizontal. Most of the joints are filled with a film of oxide of iron, but the two horizontal joints and two others are open. At the point where the vein obliquely enters the rock, the furrows on the vertical wall are continued without a break around the angle of the rock to the edge of the roof-exposure of the vein. This is plainly caused by the same joint penetrating the vein at both exposures. In general, the exposure of the vein on top of the cliff has been more unevenly eroded, and shows fewer furrows. A small piece has recently been broken from the south-east corner of the inscription tablet; and an iron-filled crack, which is found at the base of a furrow above this fracture, can be seen crossing the fresh surface, though it is faint. The inscription furrows bend downward into the two longitudinal furrows which border the so-called inscription tablet. The surrounding coarse-grained rock shows but few furrows, and they are not so regular in outline as those on the edge of the vein.

It is evident that the 'inscription' is a freak of surface erosion. The furrows are the result of weathering along joints. At the same time they differ from the ordinary weathering of the island in certain details.

A few rods from this inscription is a smaller one, very much like it in form of erosion furrows; and I found a small slab, near the north-east angle of Monhegan island, showing almost identical V and X forms.

Portland, July 27. G. H. STONE.

Recent contributions to the literature of micro-biology.

Two works upon this subject have recently been published. Dr. Friedländer, pathologist to Friedrichshain, has reviewed the French work of Cornil and Babes mercilessly, and with a personal animus not in harmony with scientific accuracy. It will be remembered, that Dr. Babes was the Hungarian authority who bitterly opposed Koch's views of the tubercular bacillus, and sought to substitute in lieu thereof 'Babes' granules.' He was, for a short time, a student in the laboratory of Professor v. Recklenhausen, and then went to Dr. Cornil, in Paris. Later, he came to Prof. Dr. Virchow, in Berlin, where he has remained ever since. He is still a very young man; and while he has not the extended experience in pathological mycology of Friedländer, Koch, or Hirschberg, he has been constant, in season and out of season, in his lab-

oratory investigations, and may justly claim a voice in this special department. It is to be regretted, that Dr. Babes concedes the primary discovery of the specific bacilli of different morbid processes to Babes alone. 'L'un de nous' creeps into the various chapters with a frequency not in harmony with accepted facts. So far as is known to microscopists, Dr. Babes has made *no* original discoveries; and the work is valuable for reference only (and in this particular its worth may not be overestimated), and as a fresh proof of Professor Cornil's facility as a writer. Lustgarten has priority in the discovery of the contagium vivum of syphilis, as well as in the peculiar process of staining. I saw Babes make several ineffectual attempts to carry out Lustgarten's directions, even while his book was going through the press; so that his statements in this connection, as well as those that occur in the discussion of actinomycosis, are purely imaginative. Actinomycoses have been successfully colored by only one man in Berlin, but his name was not Babes. The drawings in some cases are pretty good. The tube-drawings are, however, wretched, and convey an entirely erroneous impression of the growth of bacilli. Cornil's work in the book is without spot or blemish, and it is unfortunate that his duties as minister of public instruction did not allow him to give more attention to the details. Drs. G. Sims Woodhead, and Arthur W. Hare, have brought out a book jointly ('Pathological mycology'). Dr. Woodhead came to Berlin for a few weeks, worked in the laboratories, and then returned to Scotland, and wrote a book. The nature and scientific value of this publication may be estimated from the length of time which was given to the study of micro-biology. The description of methods is entirely out of date. The illustrations are singularly inaccurate, notably No. v., and all of the potato-drawings. No. 22 is not used by Koch at all, and in No. 34 the tubes are not held properly. No. 37, with description, is absolutely wrong. It is evidently a *contaminated*, and not a *pure*, culture. There is no detailed account of *drop* culture or of plate culture, which is the very basis of Koch's method of pure culture. The bacillus of blue milk forms a *brown* tint, and not a '*green*' tint, as the authors claim.

Neither of these works finds great favor among scientific men in Germany, and neither conveys any adequate impression of the exact processes of inquiry necessary to a comprehensive, intelligent survey of micro-biology. Even Koch himself stands but yet upon the threshold, working his way into the clear light of truth through much tribulation and scepticism; and even he would never dare to pronounce with such autocracy upon certain processes, as do those whose enthusiasm leads them to snap judgments after a few weeks of special study.

An interesting matter lately happened in Professor Johnne's laboratory at Dresden. A friend of mine, working up the micro-organisms of different earths, took a specimen from underneath the laboratory window. From this he cultivated some specimens of the bacilli of anthrax. Inquiry showed that formerly this place had been used as a burial spot for sheep dying of anthrax, but that for *ten* years it has not been used for such a purpose. HORATIO R. BIGELOW, M.D.

Bastei, Sächs. Schweiz, July 13.

[Our correspondent makes some strong statements which need modification. The animus of Friedländer's criticism of Cornil and Babes' book (*Fortschritte der medecin*, July 1, 1885) may be easily understood, and loses value by so much. So far as our perusal of 'Les bacteries' has informed us, the 'L'un de nous,' spoken of in our letter, refers purely to *confirmatory* work done by one or the other of the authors, and is not a claim to originality. To our thinking, Lustgarten, being the only one mentioned at all in connection with syphilis in the classification of the schizomycetes, receives all the credit the most grasping could desire: and the fact that Babes failed once or twice to stain the bacilli, proves nothing in regard to his success at other times (as any practical worker knows); nor, so far as we can see, has it any bearing upon his assertions in regard to actinomycosis. In regard to the staining of the fungus of the latter, we would suggest that some others than the one successful worker in Berlin should try washing the sections for a short time in dilute hydrochloric acid, and then stain according to Gram's method. We fancy there will be no difficulty in finding the fungus stained blue, as was demonstrated in Washington last April. Our review (*Science*, July 24) gives our own opinion of the work. Of Sims and Woodhead's 'Pathological mycology,' we have received only the first part as yet; and we have therefore not spoken of it in detail. Bad as our correspondent seems to think it, it promises to be at least the best work upon the subject that has yet appeared in English. — ED.]

'Color associations.'

Another curious phase of color association, besides the interesting one mentioned by Dr. C. S. Minot, is that in connection with names.

I have heard three children of different temperaments in the same family avow an association of colors with names. Strangely enough, they agreed on nearly every example; as, for instance, that Kate was red; Mary, white; Alice, violet; Dick, deep Vandyke brown; William, a watery blue, etc. This seems even more arbitrary and unaccountable than color associations with months; as that might, to some extent, be influenced by the prevailing tints of natural objects at those particular seasons. Thus the tone of sunlight during January, February, and March, determining the color associated, shining white yellow; that of the April sky, when there is otherwise an absence of striking color; the leading hue of Mayflowers; the zenith of verdure in June, — all may assist in forming the color associations. I may add, I know the use of color-symbols for names to exist also in adult minds in a less definite degree (the agreement between different persons also not so unanimous), but quite sufficiently to cause a confusion in recalling names of the 'same color;' as, for example, Martha and Mary Ann, both being classified as 'brownish drab.' I think if this connection of ideas were traced to the root, it would result in the conclusion that the assortment is conducted on a very elementary basis; as in the case of the two last-mentioned names, usually belonging to persons engaged in ordinary work-a-day pursuits, they are represented, or rather produce an identical effect of commonplace neutrality upon the mind, with the tint commonly adapted to serviceable uses. It is probable that thought is much more frequently carried on by hieroglyphics of form and color than by words. In fact, these afford too slow a presentation of ideas, while some faintly defined symbol conveys the effect of whole sentences at an instant. As Ribot explains a certain illusion of memory: 'there is a ground of resemblance quickly perceived between the two impressions, which leads us to identify them.' We confuse similar modifications of the nerve elements as the pictures on two slides passing simultaneously through the magic lantern are combined.

K. A. CHIPMAN.

6 Place d'Armes Square, Montreal, Aug. 3.

IMMORTALITY IN MODERN THOUGHT.

It will be admitted, we think, that the tendency of modern science is materialistic. This is especially true of biology. In fact, to many the doctrine of correlation of vital with physical forces, and the doctrine of derivative origin of species, seem little short of a demonstration of materialism. Thus materialism has become a fashion of thought; but, like all fashions, it has run into excess, which must be followed by reaction. We believe the reaction has already commenced. Science sees now, more clearly than ever before, its own limits. It acknowledges its impotence to bridge the chasm between the physical and the psychical. We pass from physical to chemical, and from chemical to vital, without break. All is motion, and nothing more; also, in the region of the vital, we pass from sense-impression through nerve-thrill to brain-changes, and still we find only motions. But when, just here, there emerge consciousness, thought, will, the relation of these to brain-changes is just as unimaginable as the appearance of the genie when Aladdin's lamp is rubbed.

It is impossible to emphasize this point too strongly. Suppose a living brain be exposed to an observer with infinitely perfect senses. Such an observer would see, could see, only molecular movements. But the subject knows nothing of all this. His experiences are of a totally different order; viz., consciousness, thought, etc. Viewed from the *outside*, there is nothing but motions; viewed from the *inside*, nothing but thought, etc., — from the one side, only material phenomena; from the other, only psychical phenomena. May we not generalize this fact? May we not extend it to nature also? From the *outside* we find nothing but motion. On the *inside* there must be consciousness, thought,[1] etc.: in a word, God. To bridge this chasm, whether in nature or in the brain, Science is impotent. As to what is on the other side of material phenomena, she is *agnostic*, but cannot be materialistic.

Admitting, then, in man a world of phenomena, which cannot be construed in terms of motion, and which for convenience we group under the name of 'spirit,' is the group permanent? Is the spirit immortal? On this subject, Science can say absolutely nothing. The field is therefore open for evidence from any quarter, and of any degree. Some of these evidences, though not given by Science, are at least suggested by lines of scientific thought. A few of these we briefly mention.

1. We have said that consciousness and thought lie behind material phenomena, in nature and in the human brain. In the one case we call it God, the divine Spirit; in the other, the spirit of man. Now, does not this identity, or similarity of relation to material phenomena, imply, or at least suggest, *similarity of nature*, and therefore immortality for the spirit of man?

2. Individual human life passes through its little cycle of changes, and quickly closes in death. If this be all, then *for the individual*, when all is done, it is precisely as if he had never been. "Yes," answers the Comtist, "for the individual, but not for humanity. Every human life leaves a residuum which enters into the life and growth of humanity. It is a glorious and unselfish religion thus to merge one's self into the only true object of worship, — humanity." But, alas! the cycle of humanity also closes; and for humanity too, when all is done, it will be precisely as if it had never been. 'But the earth — the cosmos — abides.' Yes, but only a little longer. Science declares that the cycle of the cosmos must also close. And then, when all is done, after all this process of evolution reaching upward to find its completion in man, after all the yearnings, hopes, struggles, and triumphs of man, what is the outcome? It is precisely as if the cosmos had never been. It is all literally "a tale told by an idiot, full of sound and fury, signifying nothing." Not only heart, but reason, revolts against such a final outcome. If we believe that reason underlies the phenomena of the cosmos, we cannot accept such a result. We cannot believe that the cosmos has no intelligible end. But what intelligible end is there conceivable, unless something is finally attained which is not involved in a cycle, i.e., unless man is immortal?

3. There are three primary divisions of our psychical nature; viz., sense, intellect, and will. There are three corresponding processes in making a complete rational philosophy: viz., (1) instreaming of impressions of the external world through the senses (facts); (2) elaboration of these into a consistent whole by the intellect (knowledge); (3) outgoing of this knowledge in activity (conduct). Now, a true working theory of life must satisfy all these. But scientific men are apt to think that only (1) and (2) are necessary; that true facts elaborated into consistent theory is all we need care for. Theologians, on the contrary, seem to

[1] This thought is admirably presented by Johnstone Stoney, *Nature*, vol. xxxi. p. 422.

think only (2) and (3) necessary: they elaborate a theory of life consistent with itself, and apparently satisfactory in its application to conduct, but are less careful to test its harmony with facts derived from the senses. But all three are necessary.[1] The first furnishes material; the second constructs the building; the third tests its suitableness for human habitation. All admit that successful application to art is the best test of true theory. But conduct is the art corresponding to our theory of life, and therefore the *test of its truth*. Now, is not immortality as an element of our theory of life in the highest degree conducive of right conduct? Is it not a useful, yea a necessary, element in a working hypothesis?

4. But it may be objected, animals, too, have brains: in them, too, we find evidences of something like consciousness and thought. Are they, too, immortal? If so, where shall we stop? We pass down by sliding scale, without break, to the lowest verge of life. Shall we stop here? No: for vital is transmutable into physical forces. Thus all is immortal, or none. Thus hope of immortality vanishes, as it were, by evaporation.

This objection, though serious, is, we think, not fatal. To make our view clear, we use an illustration taken from biology. May we not imagine that in animals spirit is in embryo in the womb of Nature, unconscious of self, and incapable of independent life; and that in man it came to birth, — a separate spirit, — individual, conscious of self, and capable of independent life, on a new and higher plane? According to this view, geological time is the period of gestation, evolution is the process of development, and the appearance of man the act of birth.[2] JOSEPH LE CONTE.

THE BRITISH MUSEUM OF NATURAL HISTORY.

THE visitor to the west end of London is confronted, upon turning into Cromwell Road, by a large and majestic building, whose architectural grace and warm color make a very pleasing impression upon the eye. This recent addition to the splendors of the West End is the home of the natural-history departments of the British museum. By its completion the plans of certain prominent English naturalists are happily consummated. As early as 1854 Dr. Edward Gray, alarmed by the rapid growth of the national collection of objects from the three kingdoms of nature, memorialized the trustees of the British museum upon the necessity of better accommodations. In 1862 the matter received careful attention from Professor (now Sir) Richard Owen, who published an elaborate essay upon the proper scope of a national natural-history museum, in which he presented plans for the division of material, and the erection of a museum building. These and other plans were thoroughly discussed by the naturalists of England, and the critics became eventually divided into two opposing factions, — the one maintaining that it was best to hold the natural-history collections in Great Russell Street by an enlargement of the original edifice; and the other, that it was more desirable to erect a new building somewhere in the western part of the metropolis, where more air and a better light could be obtained. The latter view finally prevailed in the government councils; but, by reason of a combination of unfortunate circumstances, nothing was done toward the erection of a new building for nearly twenty years. The collections were not moved from Great Russell Street until the autumn of 1880.

The new building stands upon a part of the ground allotted to the great industrial exhibition of 1851. Near it are the South Kensington and Indian museums, and the structures occupied by transient displays, such as the recent fisheries and hygienic exhibitions. The main portion of the building faces Cromwell Road, and presents a frontage of about six hundred and fifty feet. The two central towers are flanked on either side by a long wing and a terminal pavilion. The wings are three stories high, with a basement. The style of architecture is Norman-Gothic, richly ornamented with animal forms and conventional figures drawn from animate objects. At the back of the principal part of the structure are a number of single-storied annexes, running out at right angles to the main wall. Light for the rooms at the front and sides is obtained through large windows reaching down to the floor, but the annexes are lighted from the top.

The entire building is constructed of a buff-colored terra-cotta, which, as already intimated, is elaborately modelled, especially about the windows and doorways. The walls of the interior are likewise ornamented with conventional figures in relief. The ceiling of the central hall, presently to be mentioned, is inlaid with wooden panels upon which are painted representations of different species of plants in life-colors. The floor is a rich marble mosaic.

[1] Reflex action and theism. WILLIAM JAMES. *Unitarian review* for November, 1881.
[2] *Princeton review* for November, 1878.

The main entrance leads into the great central hall, a hundred and fifty feet long, ninety-seven feet wide, and about sixty feet high, lighted by windows near the roof, and having a gallery on the sides at the level of the second story, reached by a grand staircase at the back. The ground-floor of this hall is occupied by the index collection, which is lodged in twelve arched alcoves on the east and west sides. It also gives room to a great sperm-whale skeleton, which is the first specimen one sees upon entering the building.

Back of the central hall is another somewhat smaller, — ninety-seven feet by seventy feet, — which will be devoted to the collection illustrative of the British fauna.

On the west side of the central hall is the entrance to the bird gallery, which occupies the entire first story of the west wing and terminal pavilion. The wing is two hundred and thirty-three feet long and fifty feet wide; the pavilion, sixty feet by forty feet. The east wing and pavilion, which are of like proportions, are occupied by the fossil mammalia. Between the back wall of the wings and the annexes previously mentioned is a long narrow corridor lighted from above. The western corridor is occupied by coelenterates and sponges, and the eastern by fossil reptiles. Each of the annexes is occupied by a single group. The most westerly room contains mollusks, after which follow echinoderms, reptiles, crustaceans, and fishes. The annexes at the east of the central hall contain fossil fishes and various groups of fossil invertebrates. It will be perceived that the eastern half of the first story is devoted to fossil animals, while the western half is occupied by a portion of the collection of recent animals. The second story of the west wing is given up to mammals, and that of the east wing to minerals. In the third story (which is reached by a bridge) the west hall contains the collection of mammalian osteology, and the east hall the plants.

The basements are principally devoted to work-rooms and storage-rooms for duplicates and supplies; but at the extreme western end a cetacean gallery has been established, in which the entire collection of whale skeletons has been brought together.

There is one commodious apartment on the ground-floor which deserves special mention. It is designated as the students' room. Persons who have obtained permission to study in the museum can have brought into this room such specimens as they wish to examine. Tables and other conveniences are provided, so that investigations can be carried on with a very considerable degree of comfort. This system must commend itself to the officers of all large public museums, and to the students who resort to them.

It is manifestly impossible, within the limits of this article, to describe the cases used in the museum, or the modes of arranging specimens. Suffice it to say that the former are elaborate and costly, and appear to have been designed with much care, and that the latter are in most cases not only highly instructive, but artistic.

The chief excellence of the new structure lies in the series of annexes or galleries lighted from the top, and devoted to single groups of forms. This arrangement is in some sort an extension of the system of alcoves employed in numerous museums, but is greatly superior to the latter, on account of the size of the rooms and their complete isolation.

It is perhaps ungracious, where so much is admirable, to call attention to features which have the semblance of defects. Nevertheless, the building has been severely criticised by English naturalists, both on account of its architectural elaborateness and the faulty arrangement of its parts. The arrangement of the staircases is such as to occasion much unnecessary walking; and there is no way by which to move large specimens from the lower to the upper stories. The light in the alcoves of the central hall, devoted to the index collection, is insufficient, while along the entire southern façade it is admitted in such excess as to surely prove ruinous to the mounted specimens in the cases between the windows. There is no regular provision for a library.

It should be remembered, however, that no perfect structure was ever erected, and that the defects of this building are lost sight of in its general excellence. Its superiority over the old quarters at Great Russell Street is so great as to make comparison impossible.

The museum in Cromwell Road has somewhat more than a third more available floor-space than our national museum at Washington; but the capacity of the latter can be greatly increased by the addition of galleries. We are here comparing, however, a *part* of the British national museum with the whole of our own; for while the building at South Kensington is intended to contain only animals, plants, and minerals, the museum at Washington holds all the collections of the government illustrative of the three kingdoms of nature, and in addition those representing the history of the progress of culture and the arts.

After much agitation, the control of the natural-history collections has passed from the

MAIN FAÇADE OF THE NEW BRITISH MUSEUM OF NATURAL HISTORY ON CROMWELL ROAD.

hands of the principal librarian of the British museum to the newly appointed superintendent, who is styled director. The names of the keepers of the several sections are so well known as scarcely to need repetition. The staff, as now constituted, is as follows: —

Prof. W. H. Flower, director; Dr. Albert Günther, keeper of zoölogy; Arthur G. Butler, Esq., assistant keeper of zoölogy; Dr. H. Woodward, keeper of geology; R. Etheridge, Esq., assistant keeper of geology; L. Fletcher, Esq., keeper of mineralogy; W. Carruthers, keeper of botany. FREDERICK W. TRUE.

VEGETABLE MORPHOLOGY A CENTURY AGO. — GOETHE.

IN a previous article we have seen the conclusions reached in matters morphological by Linné and his contemporaries. Thirty years from the time of Wolff and Linné had passed away, when the appearance of Goethe's treatise on metamorphosis gave to the world once for all the true solution of Nature's problem, and, as becomes more and more apparent, determined for the nineteenth century the trend of its scientific thought. Goethe approached and stated the whole question anew; worked it out in his own persistent way; set forth with clearness the truths dimly hinted by Linné, by Wolff vainly declared; and by the splendor of his genius, and his attainments in matters purely literary, compelled the recognition of the world.

Goethe's discovery was a wonder to men of his day, is a wonder still. It is thought that the truth came to him by strange intuition, by special inspiration of some mysterious sort. His mind so surpassed that of ordinary men of talent that to his clear vision nought but truth appeared; as to a Newton, the propositions of simple geometry came without necessity of proof. But such was by no means Goethe's experience. Surely his imaginative genius suggested the idea involved; but the exposition of his theory came after months of laborious investigation, and observations repeated again and again. Furthermore, while the result proves that he made a most 'scientific use' of his imagination, it is also apparent that the poetic use of that faculty is never quite absent from his work. If his testing by observation the suggestions of his imagination is scientific, his fondness for generalization, his instinctive conviction of the unity of natural forms, and many of the details of his theory, are poetic in the extreme.

Goethe wrote of metamorphosis. The term 'morphology' does not appear in his writings until 1807. He uses the former word, however, to denote, not the actual conversion of any one organ into any other, but simply the correspondence of all organs discussed to one and the same ideal type.

In setting forth his theory, Goethe begins with the cotyledons, and shows them to be leaves after their fashion, differing in form from the ordinary leaf of the plant as they differ in function. He also notices that the first true leaves put forth are likewise not perfect, but are usually much narrower and simpler every way than those that follow. Goethe believes that the varying form of the leaf is due to variable nourishment. He regards the sap which nourishes these early leaves as very crude and poor indeed. He observes that the successive nodes of the stem receive each its sap through the medium of those below, and so each receives and furnishes to leaf and bud purer and more refined juices. Further, while the more imperfect fluids are constantly discarded, the purer are as continually assimilated and used, until Nature reaches her prescribed limits of growth: the leaves attain their greatest extent and perfection in development, and all is prepared for a new phenomenon.

Linné had shown that abundant bloom comes from scant nourishment. Goethe reasserts this, and argues, that, so long as raw material is to be disposed of, so long must all possible organs of the plant be converted into tools for the purpose. If too much nourishment is provided, the condition of blooming becomes impossible. Withdraw the nutrition, the organs of the node become more refined; the elaboration of juices unadulterated, purer and more refined takes place; the metamorphosis of the parts becomes possible, and forthwith ensues. When, in the light of present knowledge, we reflect upon the cost at which any plant puts forth its bloom, these century-old surmises of the poet seem to acquire new meaning and most peculiar interest.

In Goethe's time the involucre of a composite flower was still regarded as calyx, and acordingly the gradation from bracts to sepals was easy enough. This mistake does not, however, vitiate the poet's argument on this point; for he asserts the same transition in other plants, and cites the Compositae simply as affording the most patent illustration. He argues with perfect clearness, that the assembling about one point of several leaves, nay, even of several nodes, is not strange, as the same thing may be meetin every plumule. The

union of these narrowed sepal-leaves in the monopetalous calyx is induced by the highly refined sap circulating in a part of the plant so distal, so remote. "In the calyx, Nature produces nothing new. She simply unites and changes parts already familiar, and so rises by one step nearer her destination."

Now, as the calyx owes its character to the refinements of organs preceding it, so it in turn becomes an organ of filtration; and from the crowded vessels of the sepal comes the pure liquid which makes possible the petal. Colored petals indicate that the nourishing fluid has reached a high degree of refinement, white, of absolute purity. The equivalency of sepal and petal is shown by the usual comparison, Goethe adding only that as the sepal evinces contraction, so does the petal expansion, and we are thus prepared for the last degree of contraction in the formation of the stamen. The foliar nature of the latter organ, as of the carpels, is shown in the familiar way. The petaloid style of the iris is cited, as also the reversion so common among crowfoots, tulips, etc.

But it is to the nectary (a very comprehensive term in Goethe's time) that the poet here gives chief attention. Nectaries seem to occupy an intermediate place between stamens and petals, now partaking of the character of one organ, now of the other. Goethe inclines to the opinion that nectaries are undeveloped stamens. He is certain, at least, that the nectar itself is concerned in the fertilization of the flower; for, "after fertilization, the nectar disappears, and no more is formed." He conceived that the nectar might be an imperfect fertilizing fluid; that the pollen-grains are so many minute vesicles containing an exceedingly delicate matter elaborated by the vessels of the stamen, and destined to be absorbed by the pistil.

Think of all this in view of the modern doctrine of fertilization and cross-fertilization, and you begin to appreciate the inventive genius of the man, hovering about his subject, and almost divining that which he could not clearly see, and then his conclusion: "The forms of plants about us are not originally determined and invariably fixed, but unite with some persistence of generic, specific, and individual character, a fortunate power to vary and to accommodate themselves to circumstances, so as to be able to meet and adapt themselves to the varying conditions which come upon the earth." Darwin might have written it: Darwin could have said no more.

Such is a brief outline of Goethe's contribution to botanical science. The importance of his discovery can scarce be over-estimated, while its beauty can be appreciated only after careful examination, not only of the discovery itself, but likewise of the manner of its attainment. As to the latter, we are happily not left in doubt. The poet has himself given us a concise account of every step of his progress. We may see the impassioned enthusiasm of Linné stimulating society universal, so that Goethe says it fairly 'floated him along.' What an era in which to live, — the very springtime of science, the air redolent of odors of the life to be! Even petulant, passionate Rousseau forgets for the nonce his dreams of social compact, and, charmed with the beauty of the living world, becomes botanist, and even begins a botanical dictionary. The spirit of investigation was abroad, and Goethe entered his morphological studies with all the energy of his nature. His ideas took possession of him. The voice of Nature cried: he could not choose but hear. His work became a passion, a *leidenschaft* he calls it, from which even the marbles of Italy and the palaces of the 'eternal city' could not divert him. He was a poet; but he suddenly found out that Nature, too, is poetic, and that even her most gifted child has nothing that he has not received, nor has so much that he may not crave and receive the more. The facts of Nature were before him: his thought responded to the thought in Nature. It seemed as if it were so. It must be so. It was so.

It is said that each individual lives in epitome the life-history of his race. May it not be said that in this brief sketch of the rise of a single scientific doctrine we have found those agencies which make possible all and every science, — the light of the eye, the light of the intellect, and the vivid brilliancy of the imagination, — Linné, Wolff, Goethe, — the systematist, the student, and the poet?

T. H. McBRIDE.

AN ATTEMPT TO PHOTOGRAPH THE SOLAR CORONA WITHOUT AN ECLIPSE.

Since writing my last communication on this subject, I have made a series of investigations with the object of improving my apparatus, if possible, and of obtaining some quantitative measurements of the light reflected by the atmosphere near the sun. To avoid the reflection of the light from the surfaces of the glass, I removed my achromatic lens, and substituted for it a simple spectacle-glass of one and three-eighths inches diameter, and forty-nine inches focus. As the diameter was relatively so small, the

inclination of the surfaces to one another at any point was necessarily very slight; and this fact, combined with the extreme thinness of the glass, rendered the multiple internal reflections almost entirely harmless. Five diaphragms were used between the lens and plate, cutting off totally all internal reflection from the tube. The same drop-shutter was used as before, working just in front of the lens.

Several observers have attempted to compare the brilliancy of the corona as seen during a total solar eclipse with that of the full moon. Thus Belli in 1842, and Wilson in 1860, find the corona brighter than the moon; while Halley in 1715, Billerbeck in 1851, and Curtis in 1869, find the moon brighter than the corona.[1] Other observations by W. O. Ross in 1870, and by J. C. Smith in 1878,[2] would indicate that the corona was somewhat brighter than the full moon.

A photograph of the solar corona in order to be of any use whatever should show something more than a thin uniform ring of light around the sun. It should show some structure, some details of the rays and rifts visible at the time of an eclipse. The only observations which I have been able to find of the intrinsic brightness of different portions of the corona, were those by Professor John W. Langley in 1878.[3] He found that the corona at 1′ distance from the sun had a brilliancy equal to six full moons, while at 4′ distance it was only equal to .1 of a full moon. Unfortunately for our present purpose these observations were visual, and not photographic; but they will give us an idea of the size of the figures with which we are dealing. In order, then, for a photographic plate to show any of the irregularities of detail in the corona, such as the contrast between a ray and one of the neighboring rifts, it must be capable of showing contrasts of light which do not vary from one another by more than about one-tenth the intrinsic brilliancy of the full moon.

A series of investigations was next instituted to determine what excess of brilliancy it was necessary for one surface to have over another, in order that the contrast between them might be rendered perceptible in a photograph. The difference of brilliancy required by an acute eye amounts to between one and two per cent. One sees it sometimes stated that the camera is capable of distinguishing contrasts which are quite invisible to the eye. This, I think, is a serious error. If both sources of light are of great brilliancy, though differing considerably, the eye may not perceive a very great difference between them; while a negative with a very short exposure may show a very considerable difference. If, however, the eye be protected by colored glasses, the contrast will be as great as that presented by the negative.

But the question which occurs at present is not where great differences of light exist, but where the differences are very small, of only a few per cent. The inferiority of the best gelatine plates to the human eye in this respect is very readily shown by an attempt to photograph distant mountains. It will be found that it is perfectly impossible, even in the clearest weather, to photograph at a much greater distance than fifty or sixty miles. I recently, on a very clear morning, made a mountain ascent with a camera. Mountains over ninety miles distant were readily recognized and distinguished; but, on pointing the camera in their direction, nothing over forty miles distant could be photographed. There was no question but that mountains at a much greater distance than ninety miles could have been seen, had there been any high enough to be visible. Every photographer who has visited mountain regions is perfectly aware of the disabilities under which he labors in this respect.

Another illustration of the same thing is the impossibility of photographing the moon in the daytime, when the sun is high above the horizon. Although the moon may be perfectly distinct to the eye, the negative shows no trace of it. This fact of itself, I think, has a direct bearing on the question in point.

But in addition to these general facts it was thought that some quantitative results would be desirable. Besides the chloride plates which I had been using, several well-known kinds of bromide plates were tested at the same time. These were selected with especial regard to the strong contrast qualities which they were supposed to possess. The plates tested were the Anthony chloride, the Carbutt B, the Allen and Rowell, and the Stanley. Different portions of the plate were exposed to a uniform illumination for various times, and it was found that all the plates gave about the same result, and that if the division lines between the areas were very sharp, and over an inch in length, as small a contrast as five per cent could be detected; but if the division lines were not over one-eighth of an inch in length, even if one knew just where to look for them, it was impossible to recognize a difference of less than ten per cent upon the negative. As the coronal rays on the photograph would be less than one-eighth of an inch in length in order to reach out beyond 3′, ten per cent was selected as the limit of contrast necessary to obtain a satisfactory result.

Since the light reflected by the corona at 3′ distance from the sun is only .1 that of the full moon, in order to distinguish between a coronal ray and a neighboring rift at that distance, it is necessary that the light reflected from the earth's atmosphere in that region should not exceed in intrinsic brilliancy that reflected by the moon itself.

A series of observations was next made to determine the relative light of the sun and of the sky in its immediate vicinity. The method employed was as follows: Half of the photographic plate was covered with thick yellow paper; a diaphragm of .016 centimeter in diameter was placed in front of the lens, and four different exposures made to the sun on different parts of the plate, lasting respectively for two, four, eight, and sixteen seconds. The plate was then taken into the dark room, and the exposed portion protected by the yellow paper, which was re-

[1] *Memoirs of the Royal astronomical society*, vol. xli. pp. 243-253.

[2] Washington observations, 1876: Appendix iii., p. 387.

[3] Washington observations, 1876: Appendix iii., p. 211.

moved from the other half of the plate. The telescope was now so placed that the sun should be hidden behind a paper disk, fixed at about twenty feet distant. A diaphragm of one centimeter aperture was placed in front of the lens, and an exposure of four seconds given to the sky. On development, half of the plate, except where cut by the image of the disk, was found uniformly darkened. On the other half were four images of the sun, two of which were lighter, and one darker, than the sky. The third image of eight seconds exposure was of exactly the same darkness as the sky; and it was accordingly shown, that since the diaphragm used with the sky was about four thousand times larger, the sun was about two thousand times as bright, photographically, as the sky in its immediate vicinity. A number of plates were taken on different days, when the sky seemed perfectly clear, and the results indicated that the number varied in general between a thousand and four thousand. Owing to the diffraction produced by the small diaphragm used in photographing the sun, which rendered the image 1.6 times larger than it really should be, all these figures must be multiplied by 1.6.

Comparisons were then made in a similar manner between the sky near the sun, and the full moon, the latter taken with the full aperture of the lens, 3.65 centimeters, and the former with an aperture of .204 centimeter. Under these circumstances, with exposures of fifteen seconds, the moon and sky darkened the plate to about an equal amount. The result of a number of experiments indicated that the sky in the immediate vicinity of the sun was of about four hundred times the intrinsic brilliancy of the full moon. The ratio of the sky to the sun on this same day was fifteen hundred, so that the light of the moon was to that of the sun as one to six hundred thousand. In some experiments which I made in 1879,[1] I found the visual ratio was one to three hundred and fifty thousand. On account of the extreme blueness of the sun, it was to be expected that the photographic ratio should be somewhat higher than the visual one.

I next tried comparing directly the light of the sun and moon on the same plate, in order, if possible, to get a check on my results. The results, however, were unsatisfactory, the ratio coming out as 1 to 300,000, or only one-half the former amount. Owing to the difficulties of the experiment, this discrepancy may very well be referred to inaccuracies of the photographic plate, and changes in the sun's and moon's light during the course of the experiments. In all the results with regard to the sun, it must be remembered that the figures must be multiplied by 1.6, on account of diffraction. The two ratios, then, of the light of the moon to that of the sun, stand as 1 to 960,000, and 1 to 480,000; and of these, I think, in connection with my visual result, the former is the more correct figure. The moon at the time of these observations, June 26, 12 M., had an altitude of 29°, when the atmospheric absorption would amount to about twenty per cent.[1] Making this correction, we have the photographic ratio of the moon to the sun, as 1 to 760,000, or about twice as great as that to the eye. This is, of course, only an approximate result, as only very few observations were made, and as it was entirely outside the course of our inquiry.

Returning, then, to our original subject, we found the sky near the sun 400 times as bright as the full moon. Correcting for atmospheric absorption, this figure becomes 320 times. But we found before, that in order to detect the contrast between a coronal ray and a neighboring rift, the light of the sky must not exceed that of the full moon. It therefore seems that even in the clearest weather the reflected light of the atmosphere is 300 times too strong to obtain the faintest visible image of the true coronal rays.

In connection with these experiments, I took a few photographs of the sun with my improved apparatus. In order to still further diminish the reflection of the light from the surfaces of the lens, I so placed the telescope that the sun was almost completely hidden behind the high steeple of a neighboring church. A vast improvement in the results was at once obtained. The sun stood out sharply defined on a perfectly uniform background of blue sky. There was not the slightest trace of a fringe either where the steeple crossed the disk, or where the sky came in contact with the solar limb. The day was beautifully clear, and at six in the afternoon some more photographs were taken; but now, although the steeple was as clear as ever, all around the limb of the sun appeared the atmospheric halo, extending out in all directions, and gradually growing fainter as it receded from the sun. We may, therefore, in general, say, that with properly constructed apparatus, in perfectly clear weather, no halo whatever appears around the sun. It is only in slightly hazy weather, or as the sun approaches the horizon, that the appearances are produced which have been elsewhere described.

In brief, the result of my researches would seem to indicate, 1°; that without a total eclipse it ought to be impossible to photograph the solar corona, 2°; having tried, I have failed to photograph the corona, but have obtained the result which theory indicated.

WM. H. PICKERING.

[1] *Proceedings of the American academy of arts and sciences*, 1880, p. 246.

STEINEN'S EXPLORATIONS OF THE XINGU.

DR. KARL VON STEINEN has recently made a most interesting report of his explorations in the Matto Grosso,— the immense region, more than four times as large as France, which occupies a large part of central and western Brazil, and is hardly known to geographers except in the most imperfect manner. It is divided by great rivers, of which the Madeira, the Tapajos, the Xingu, the Araguaya, and the Tocantins flow northward, and the Paraguay flows southward. It is watered by innumerable streams which unite with these rivers, along whose banks live thou-

[1] *Annals Harvard college observatory*, vol. xiv. p. 62.

sands of the most primitive human beings. Steam-navigation exists on the Tapajos and Tocantins, and a railway is constructing along the banks of the Madeira. The Xingu is, however, almost unknown. Previous explorers have not reached beyond 4° south latitude, beyond which were supposed to be fierce tribes of cannibals. The primitive inhabitants of Brazil, retiring before the whites, were supposed to have concentrated themselves here as in a last stronghold. Between Piranhaquara and Paranatinga the river was absolutely unknown; and the existence of a water-way for trade to Para is a matter of the utmost importance to the inhabitants of the Matto Grosso, now confined for commercial purposes to the Paraguay.

The first part of the journey was made by land, and the latter part on the river. The military force commanded by Paula Castro worked with the civilians in perfect accord. They left Cayaba May 26, passing through the region of the Baicairis (partially civilized and friendly Indians), and after some delays, caused by the fact that the real distance was one-half greater than shown on the maps, they crossed the Paranatinga July 5, 1884, entering the unknown region, and travelling westward. The plain is a vast desert with an altitude of some twelve hundred feet, above which rise dunes of red sand and decomposed gravel to a further height of from two hundred to three hundred feet. The water was good, the grass very tall, but trees scarce and low, the vegetation being chiefly near the streams. Tapir and deer were seen, and a few birds, but the fauna is scanty. At mid-day the temperature was 80° F., but during the night it fell to 45°, and the party found great difficulty in obtaining game enough for food. Twenty leagues east from Paranatinga they found a river which they decided must be the Xingu. Making bark canoes, they began to descend it, meeting great difficulties: the river seemed to contain more rolling stones than water. In nineteen days, when they met the first Indians, they had passed more than a hundred rapids and four cataracts, of which one was fifteen feet high. Seven canoes were destroyed or wrecked: only six remained. Their provisions were almost exhausted, their clothing in rags, shoes worn out, and the men depressed by Malaria and the labor of frequent portages, when everthing had to be carried on their shoulders around rapids. Aug. 30 they arrived at the mouth the Batovi, and had reached more level country. Here three large rivers unite to form the Xingu, which is about a quarter of a mile wide. Numerous distinct tribes are located in this vicinity, all in about the same stage of culture. Surprised by the advent of the whites, they offered no hostilities, though fierce and untamed. They knew nothing of fire-arms. The reflection of the sun by a mirror alarmed them. One band of Suyas proposed a joint expedition against the Trumais, with whom they were at war. The Trumais live in villages of high round huts, several families in each. They cultivate manioc, maize, potatoes, and cotton, smoke wild tobacco, but do not know the banana. They do not hunt much, but shoot fish with arrows, and net them in pools. They have no spears, but kill wild animals with bow and arrow. They regard the flesh of the capybara as a delicacy, but do not eat that of the tapir or deer. Monkeys are eaten, their flesh dried and smoked for future use. They were much afraid of the dogs with the expedition: only one tribe had a name for this animal. The men go naked; they wear, however, strings of ornaments, teeth, shells, or nuts around the neck and waist, and a sort of cotton ribbon on the arms and legs. The women of most tribes wear a clout made of palm bark which could be put in a match-box. The Suya women wear absolutely nothing, although they know how to weave cotton hammocks, and make a sort of cordage of vegetable fibre. They do not know metals: all their tools are of bone and stone. Buttons were extremely desired. Steinen thought that with a gross of buttons he could have bought a house, field, food, and several wives. They are well proportioned. They practise the tonsure, shaving the crown with a very hard, sharp leaf resembling grass. They wear two feathers in their ears, and a diadem of feathers or straw. They play the native flute agreeably, and are fond of music. They ornament themselves at their feasts with bijous made of cotton or straw, or carvings of wood representing birds. In the houses are hung figures of animals coarsely plaited of straw, with some artistic merit. The Suyas, of whom only about a hundred and twenty were seen, are the terror of the other tribes. They are of greater stature, though absolutely nude, and wear a labret in the lower lip, and straw ornaments in the ears which reach to the shoulder. They cut the hair in front, and leave it long behind. They make baskets, hampers, and boxes of straw, and very perfect carvings of birds in wood. Their flute has three pipes of graduated size. They fight with heavy clubs ornamented with inlaid shell.

After leaving these people, another series of difficult rapids was encountered. The party were reduced to severe straits, were obliged to live on fish, two pumas which were killed served as a delicacy, fever prevailed among them, and their clothing was almost gone. Below the rapids they encountered a friendly tribe, the Yurumas, who were entirely ignorant of the people farther up the stream. These Indians sold them new canoes, and furnished guides. At last, on the 13th of October, the weary explorers reached Piranhaquara, the first outpost of civilization, almost naked and exhausted, but without the loss of a single man.

They had demonstrated the inutility of the Xingu as a trade route, but they obtained most valuable geographical and other scientific data in their traverse of the unknown region.

GEOGRAPHICAL NOTES.

The great work of Gen. Tillo on terrestrial magnetism in European Russia, has just been published by the St. Petersburg academy of sciences. All previous investigations have been laid under contribu-

tion, and the work is accompanied by diagrams, and a magnetic chart of Russia. Investigations in this direction have been very active recently in Russia. Besides the work of Tillo, Miller, Scharnhorst, etc., Schwartz has recently published in the 'Russki invalid' important researches on the magnetics of Turkestan, especially of the observatory at Tashkent.

Charles Rabot has finished a reconnoissance of the Norwegian glacial region, known under the general name of Svartisen. This work is the result of several years' explorations, during which the author received the kindly co-operation of the Norwegian general staff. It is based on a series of triangulations, with the details filled in by means of sketches, and photographs taken at determined angles and azimuths. The result shows a region about 125 kilometres long by forty-five kilometres wide, and divided by mountains into four principalg lacial bodies, but which, on the latest charts, is shown as covered by a single dome of ice.

The Military geographical institute of Italy has published a memoir on the mensuration of the area of the kingdom, and a new essay at the same. The figures are as follows in square kilometers: —

The peninsula of Italy	236,402.1720
The islets legally connected with its shores	368.8649
Sicily	25,461.2535
The Sicilian islets	278.8147
Sardinia	23,799.5607
The Sardinian islets	277.6027
Total	286,588.3—

This is about ten thousand square kilometres less than previous official figures, and two thousand less than Gen. Stebnitski's estimate.

The ethnography of the Austrian *litorale* has been deduced by Baron Carlo von Czoernig from the census of Dec. 31, 1880. The total is six hundred and eleven thousand in round numbers, of which 45.03 per cent are Italians, 32.27 Sloveni, 20.21 Croats, 0.35 Rumanians, and 2.14 German-Austrians, and others. Ethnographically, therefore, these coasts are Slavo-Italian.

Dr. Zélandt has just finished his great work on the Kirgiz, which will be soon published by the west Siberian section of the Imperial geographical society. It is divided into seven heads, treating of the history and archeology of Semirechinsk; of the resources of the central Tian-shan; of the life of the nomadic Kirgiz; of their social, commercial, and political institutions; of their ethnic relations; and of their temperament and culture. This work is supplemented by Katanaieff's recent memoir on the progressive movement of the Kirgiz of the Middle Horde, toward the Siberian frontier. A new chart of Russian-Turkestan, scale 1:42000, has just been issued at Tashkent.

It is announced that the work on the commercial geography of China, by Isidore Hedde, has been interrupted by the illness of the author, who has devoted twenty years to it, and was formerly a commercial agent of France in China. Two volumes still remain in manuscript, and will be printed if a sufficient number of subscriptions are received by Paul Perny, care of the Société de géographie, Paris.

Dr. Ten Kate has just sailed for Surinam, with the intention of ascending the river of that name, crossing the Tumuc-kumac mountains, and descending to Brazil by the affluents of the Amazon River.

The recent expedition of Professor Chaffaujon on the Orinoco has been heard from. He had reached Caicara, and had prepared a map of the Orinoco and the region closely adjacent to its banks. In this work he was able to obtain much geological information, and discovered numerous pictorial and graphic aboriginal inscriptions, some of which seemed to be of the nature of writing. An immense mass of ethnological and natural-history collections had been made. Travelling was very expensive, and a large number of men were required to carry on the work.

Father T. Gaujon writes that Vidal Senèze, who had undertaken an exploration in the Chincha Islands, died at Guayaquil, and his collections were dispersed. The notes of his previous journey from Zumba to Bella Vista, reviewed by several residents of the region, had a certain importance; and the traveller, though without much training, had a spirit and an energy which make his death a loss to science.

A. Chaigneaux is about to take part in an expedition sent out by the Chilian government to the region where Crevaux lost his life, in Bolivia.

A CRAB INVASION.

AN interesting occurrence, that should be placed on record, has been recently reported by Mr. L. S. Foster of New York, superintendent of the Spanish American district of the American ornithologists' union. It consisted in the sudden appearance of countless myriads of young crabs on the seashore at Cape San Antonio, the western extremity of the Island of Cuba, where it was observed by Francisco Baritista y Ovenes, keeper of the lighthouse at that place. Specimens of the crabs were sent to the U. S. national museum by Mr. Foster, accompanied by the following extract from a letter by the lightkeeper, dated June 14, 1885: —

"After the light of the lighthouse had been extinguished in the morning of April 3, 1885, we went out on the gallery and saw at the edge of the shore, and at intervals farther out, large and small floating patches, of a reddish color, of what appeared to be wood, gulf-weed, or some other vegetable product of the sea. To our surprise, upon inspecting them more closely, we found these patches to consist of small living and moving bodies, belonging to the crab-family, being of that shape. I proceeded to measure the piles that were forming on the shore, and many of them exceeded one and one-half metres in size [probably diameter]. At eight o'clock in the morning, as more of the patches floated in shore, some of the piles increased to two metres. This multitude of marine animals came from the south-west, the wind and tide being from that direction; and the same phenomenon was repeated on April 9, and May 2 and

5. They approached the shore mostly during the night, the movement continuing, however, somewhat into the early morning hours. They invaded the houses and the yards, and the tower of the light-house up to a certain height, so that we had to brush them away with brooms and shovels, and finally to close the doors and windows, and cover the openings of the water-tanks with canvas and sacking. We lost three tanks of water corrupted by these little creatures. After sunrise they were nearly all killed by the heat, becoming whitish. A few that escaped to the shade lived a few days without growing any larger."

Prof. S. I. Smith of Yale College, to whom the specimens were referred, reports as follows respecting them: —

"The very small crabs from Cape San Antonio, Cuba, are too young and imperfect for precise determination, but are evidently the young, changed from the free-swimming megalops stage of some Grapsoid crab, probably a species of Sesarma. The four specimens are evidently all of the same species. They measure between four and five millimetres in width of carapax." R. RATHBUN.

TYPES OF ETHICAL THEORY.

DR. MARTINEAU (it is a pleasure to remember that this country had the honor of giving him his title) has already reached his fourscore years, yet his work shows no sign either of labor or of sorrow. Its characteristics are indeed precisely the reverse of these: they are facility and optimism. There is the same dignified eloquence which made George Eliot write, in 1853, "James Martineau transcends himself in beauty of imagery." There is the same calm faith which has always possessed him in the outcome of the philosophical controversies of the time. For forty years he has stood quite alone among English theists in his breadth of sympathy and his sweep of style; and there is much pathos to many a grateful student in the words with which he dismisses this work, hoping to deal with further problems, "in case the evening twilight of life should linger a little longer with me, and leave my powers of industry still unspent."

It is impossible to review such a book as this with any completeness, within the limits which must be here observed. It is the ripened fruit of a lifetime, and it must be recognized, as has been done by the *Spectator*, as the most important ethical work of this generation. It traces the great types of ethical theory, advancing with "many companions, stately or keen, severe or facile, mystic or humane," until the view of the author is set in final and striking contrast with that of the so-called English school. Here, to most readers, is the central interest of the book. It is Kantian ethics in the hands of a master of style over against the laborious inadequacy of Mr. Spencer. Nothing can be more delightful than the ease and brilliancy of this discussion, or more honorable than its recognition of the worth of the opposing school. "The representative writers of this school," Mr. Martineau concludes, "have in truth theorized in one language, and felt in another, and have retained ideal conceptions of a scale of good, and admirations for types of character, for which their doctrine can find no corresponding place. Nor is this an accident of their individual presentations of the theory. So long as it sets itself to find the moral in the immoral, to identify the order of right with the order of strength, to repudiate any study of what ought to be except in studying what has been, is, and will be, it totally shuts the door in the face of all conception and possibility of duty, and by naturalizing ethics reverses the idealizing process which rather ethicizes nature. It subjugates character to science, instead of freeing it into religion."

Two sources of embarrassment are here hinted at, which are felt throughout the work. The one is the loyalty of the writer to the terminology of the school in which he has been reared. This is so marked in the presentation of the author's own theory, that the hasty reader may fancy that he is dealing once more with that analysis of faculties which used to satisfy the writers on ethics, and which made the study so dreary. "The virtues and vices, the appetites, emotions, and affections," some one has said of that earlier school, "stood each in its appointed corner, and with its appropriate label. Never before had human nature been so neatly dissected, or so ornamentally packed up." It is not until one has penetrated through this somewhat repelling method, that he discovers the wealth of insight which Dr. Martineau's treatment exhibits. The other source of embarrassment is more serious. It is the obvious conviction of the writer that the principles of ethics cannot be finally described apart from their relation to religion. After all is said and done, human nature remains, as Mr. Bradley most forcibly points out in his 'Ethical studies,' a contradiction whose solution compels one to the religious attitude. Dr. Martineau constantly hints at this necessary incompleteness; and

Types of ethical theory. By JAMES MARTINEAU, D.D., LL.D. 2 vols. Oxford, *Clarendon press*, 1885.

his preface promises that the philosophy of religion shall be his next task. How the relation of ethics to faith would be developed by him may be seen in his very remarkable lecture on this subject, delivered in 1881. Here his spirit has its natural flight, unhindered by controversy or by ethical limitations.

We turn, finally, from these very insufficient suggestions of the contents of the work to a single element in it which will be novel to most readers, and interesting to all. Dr. Martineau is led in his preface to describe the personal experiences which gave its character to his work, and in so doing he offers us a most fascinating and instructive glimpse of his own intellectual autobiography. It seems that he was originally trained to be a civil engineer, and his first philosophical studies were controlled by scientific conceptions. "So self-evident appeared the maxims of mechanical causality, that in my heart I deemed it blindness if any one professed a different vision." . . . "It is no wonder, that, in skimming over my notes of work in those distant years, I seem to be communing with some tight-swathed, logical prig, in whose jerky confidence and angular mimicry I am humbled to recognize the image of myself." It was the discipline of teaching these subjects which changed his views; yet the change was not so obvious to himself as it was to his friend, J. S. Mill. "Though he saw to the bottom of my apostasy, he did not cut me off as a lost soul." Finally, under the guidance of Professor Trendelenburg and the inspiration of Greek philosophy, he gained what he describes as a 'new intellectual birth.' "It was as if the stereoscope through which I had looked at Plato or Aristotle had had its double picture, — Greek and English, — with distorted halves, producing only a blurred and overlapping flat; while now the slide of true correspondence was there, and the eye, after a momentary strain of adaptation, beheld the symmetrical reality in all its dimensions." . . . "The metaphysic of the world had come home to me; and never again could I say that phenomena, in their clusters and chains, were all." To many a student there will be nothing of more value in these volumes than these suggestions of what the author calls "the transitions of his thought, and the testing crises of his life." FRANCIS G. PEABODY.

WORK OF THE CHALLENGER EXPEDITION — III. GEOLOGICALLY VIEWED.

THAT the work of a dredging and sounding expedition should add much to our knowledge of the geology of dry land, except by inferences from submarine formations, is hardly to be expected. Nevertheless, this report contains many facts and observations useful to geologists. Several of the phototype plates are extremely striking illustrations of geological phenomena, showing more on one sheet than many pages of text would do. Such, for instance, are the plates illustrating glacial markings in Nova Scotia (i. p. 158), the trap-hills of Kerguelen (p. 338), and the wonderful lava cascade of Kilauea.

Only two of the series of special reports, actual and projected, treat of essentially geological matters; one already printed being on the petrology of St. Paul's Rocks, by Prof. A. Renard. These rocks, far removed from any continent, consist of a number of small islets separated by deep chasms, through which the ocean unceasingly pours and rises into breakers. The rock-mass, according to Professor Renard, is peridotic; and, while admitting the possibility of the volcanic origin favored by analogy, he has been led, rather, to presume that the rocks are a remnant of upheaval of an orographic character. This view has been opposed by Professor Geikie, and in this journal by Mr. Wadsworth (*Science*, i. 1883, p. 590), and would seem yet unestablished.

The second report referred to is that of Dr. Murray, on the deposits of the deep-sea bed. One of the most attractive plates in the work before us is that (p. 926) illustrating the ooze formed by the diatoms, radiolarians, foraminifera, and other organic remains on the sea-bottom, as seen under high magnification. After the removal of the calcareous portions, and the determination of the carbonic acid, the remainder is divided by Dr. Murray into mineral matter, the *débris* of siliceous organisms, and fine sediment. The material found in inland seas and along continental shores consists in large part of terrigenous deposits, the different colored muds and sand, and volcanic *débris* of inorganic origin; while corals and corallines afford sand and mud of organic origin. The abyssal deposits, on the other hand, in large part, seem to consist of ooze derived from remains of minute animals, such as pteropods, diatoms, etc., and especially of a red clay such as results from the degradation of the ooze and of decomposed pumice. The transition between the former and the latter is gradual, but in the great deeps the clay almost exclusively predominates. The terrigenous deposits reveal the equivalents of chalks, green sands, marls, or shales, but in the deep-sea deposits, according to Dr. Murray, differ pro-

foundly from the series of rocks known in the geological formations. The latter present no analogies to the red clays and oozes, in which, for instance, quartz may be said to be practically absent. The deduction from this is made, that the deeps are of great antiquity. In order to account for such vast accumulations as were there discovered, it is necessary to suppose that these basins have remained the same for a vast period of time.

From the red clays south of the equator, quantities of ear-bones of whales, sharks' teeth, etc., were obtained, which were embedded in nodules of peroxide of manganese, derived from the salts of that metal contained in the sea-water. Some products of volcanic eruptions also occurred, and, more interesting than either, certain spherules for which a cosmic origin is confidently claimed. These are mostly extremely small (not more than a millimetre or two in diameter), and may be collected from the dry and powdered ooze by a magnet. These contain sometimes a centre of meteoric iron coated with magnetic oxide, sometimes what seems to be an alloy of cobalt and nickel: others are chondritic, and appear to consist of bronzite or enstatite. All these are characteristically meteoric minerals; and it is indeed remarkable that the investigations of a Nordenskiöld in the arctic snows, should, in their proof of the gain of this planet by the deposit of cosmic material, be upheld and augmented by an investigation of the abysmal ooze of the great deep.

To the narrative are appended, among other documents, a bibliography, sufficiently exact for general purposes, of papers and publications, official and otherwise, to which the voyage has given rise. There is a list of the special reports already printed (more than forty), and of nearly as many more to follow. The concluding part will include an index to the whole, which it is to be hoped will be intrusted to a competent person for preparation. There is a science of indexing, to which we are confident the person who indexed this narrative never served an apprenticeship. Considering the importance, variety, and multitude of facts recorded in these pages, and that there is no systematic arrangement of them in the text, a really thorough, sensible, and scientific index was indispensable. The one which exists, though voluminous enough, is far from meeting the least of these requisites. In this particular, and a few others, we have, as it were, indicated a few spots upon the sun; but we should do much less than justice to the editors, and to the authorities who have sanctioned the work, were we to omit a distinct enunciation of the opinion that it, and the series it is intended to introduce, as a whole, form the most magnificent contribution to natural science, and monument of enlightened research, which has ever been given to the world in any age or by any country.

THE CHALLENGER REPORT ON THE STALKED CRINOIDS.

THIS magnificent work, which has just been issued by the British government, is beyond question the most important contribution to the literature of the living crinoids since the days of Johannes Müller. When Müller wrote his classical work, 'Ueber den bau des Pentacrinus caput medusae,' in 1841, but a single species of stalked crinoids from the existing seas was known. Carpenter now describes six genera, with thirty-two species, of which two genera and eighteen species were brought to light by the Challenger. In the present report he describes also the remarkable comatulid genus, Thaumatocrinus, obtained from a depth of eighteen hundred fathoms, which has underbasals, and interradial plates interposed between the first radials, and a row of anal plates, thus combining, in a measure, the characters of recent and paleozoic crinoids.

The work, though primarily a report upon the crinoidal collections of the Challenger expedition, is, in fact, an almost complete monograph of all recent stalked crinoids known to this time. The descriptive part and illustrations are so excellent and copious as to leave nothing to be desired in this particular.

A large portion of the volume is devoted to comparative discussions of the morphological relations between recent and ancient crinoids, to which he has brought all the resources of a mind equipped with the most varied and accurate knowledge of both living and extinct forms. The importance of this portion of the work in stimulating further researches cannot be overestimated.

In his classification, Carpenter follows Leuckart, and separates the stalked echinoderms from the remainder of the group, under the name 'Pelmatozoa,' which he makes a 'branch' of the 'phylum' Echinodermata, with three 'classes,'—Crinoidea, Cystidea, and Blastoidea. The Crinoidea are the strictly brachiate Pelmatozoa, for which Burmeister, in 1856, proposed the name 'Brachiata,' taking rank with

Report on the Crinoidea dredged by H. M. S. Challenger during the years 1873-76. Part i. General morphology, with descriptions of the stalked crinoids. By Dr. P. HERBERT CARPENTER.

the 'Anthodiata,' under which he placed the Cystidea and Blastoidea as sub-groups. The Blastoidea, no doubt, are readily separated from the true Crinoidea; but the two groups are so closely linked together by the Cystidea, that it is extremely difficult to assert whether certain forms are crinoids or cystids, or whether others are cystids or blastoids. For instance: Caryocrinus and Porocrinus have well-developed free arms, but possess calicine pores; while Hybocystites, on the contrary, has cystidean arms and no calicine pores. Similar transitions connect the Blastoidea with the Cystidea; and it is scarcely doubtful that crinoids sometimes have hydrospires. These difficulties do not seem to be wholly met by Carpenter's arrangement, nor indeed, it must be confessed, by any other as yet devised.

Dr. Carpenter's discussion of the relations of the Neocrinoidea to the Palaeocrinoidea should be studied by every paleontologist who aims at something more than mere empirical descriptions. He ranks the two groups as distinct orders, and points out very clearly their structural differences. In the course of these discussions he directs special attention to the oral plates of the Palaeocrinoidea. These plates, he thinks, are represented by the so-called 'proximals,' or ring of plates surrounding the central piece, which he regards as corresponding to the basals in the abactinal system. He calls the central plate the 'orocentral,' and considers it an actinal representative of the 'dorsocentral,' the terminal plate of the column. From one point of view, this theory appears plausible, considering that there is a dorsocentral in ophiuroids and starfishes enclosed within the ring of basals; but it is difficult to understand what function such a plate could have had in the oral system, since it is to be compared with the base of the column in crinoids, while no echinoderm, at any period of life, or in any group, was ever attached by the oral side.

The limits of this notice do not warrant further mention of the details of the book, much less discussions; but the work challenges admiration in almost every requisite of a scientific treatise. Dr. Carpenter's style is clear, vigorous, and incisive. Those who venture to cross swords with him in scientific disputation will do well to carefully measure their strength; for they will find a most formidable antagonist, fully armed at all points, vigilant to discover, and quick to strike at the weak points of an argument. With all this, it is a pleasure to observe the eminent candor and fairness of his treatment of controverted questions. Those who dispute with him are not allowed to forget that the ultimate aim of all such discussion is not a victory of words, but the discovery of the truth.

NOTES AND NEWS.

THE Chesapeake zoölogical laboratory, as the marine station maintained by the Johns Hopkins university is designated, is established for the present summer session at Beaufort, on the coast of North Carolina. Dr. W. K. Brooks, the director, who was prevented last year by ill health from giving as much time as usual to the laboratory, is fortunately quite restored to his usual strength, and is in full activity at his post. Twelve collaborators are with him, — Messrs. Andrews, Bruce, Haldeman, Hemmeter, Herrick, Howell, Jenkins, McMurrich, Mills, Morrell, Nachtrieb, and Shimek. Several of these are already teachers in various branches of zoölogical science, and all of them are well prepared to make use of the opportunites which are afforded at this station. An unusual number are engaged in original researches. On account of the limited accommodations, the director was unable to receive three other persons who applied for admission. The season of 1885, although uncomfortably hot, has thus far been exceptionally favorable for collection. The weather has been calmer than heretofore in June and July, and specimens were found in June which have usually not appeared until the middle of August. The company, notwithstanding their personal discomfort from the heat, have maintained their full enthusiasm in the work upon which they are engaged; and it now appears as if the eighth session of the laboratory would be more fruitful in results than its predecessors, good as they have been. It is too early to speak of the investigations which are in progress, but reports will be made upon them in one of the Johns Hopkins university circulars to be published in the early autumn.

— A cable message to Harvard college observatory, from Dr. Krueger, at Kiel, announces the discovery at Nice of Tuttle's comet (1858) on its expected return. The position received is as follows: August, 9 6124*d.*, *Gr. M. T.; R. A.*, 7*h.* 23*m.* 43.1*s.; Decl.*, +28° 1′ 24″.

— Prof. J. E. Hilgard, who has just resigned from his position as superintendent of the U. S. coast-survey, was born in Zweibrücken, Germany, in 1825. His father, a lawyer by profession, emigrated to Illinois in 1835, with a family of nine children, and was a man well known for his writings on social questions. J. E. Hilgard was educated as a civil engineer, and in 1845 entered the coast-survey: he was specially interested in geodetic methods and the tides and terrestrial magnetism. In 1863 he was made a member of the National academy of science, and in 1872 took an active part in the international metric commission in Paris, and was made one of its permanent committee; and it was in that year he

made a determination of the difference in longitude of Cambridge and Greenwich by means of the Atlantic cable. At his suggestion, the series of transcontinental triangulations have been run connecting the surveys on the Atlantic and Pacific coasts, — triangulations which are serving for some of the state-surveys now in progress. Professor Hilgard was especially interested in the study of the Gulf Stream; and many will remember his paper upon that subject read at Philadelphia last summer, but not yet published. The success of the coast-survey is largely due to his work; and, since 1881, he has been the superintendent, from which position the misfortunes of ill-health compelled him to resign.

— Mr. Bosworth Smith has been appointed mineralogist for the Madras presidency. The local government order says he is "to create in the Central museum a perfect index to the mineral wealth of the presidency, and to begin a mineralogical survey in consultation with Dr. Bidie and such other officers as government may instruct him to communicate with."

— The commander of the German gunboat Prinz Adelbert reports that on the 10th of February last, at half-past one A.M., in the roadstead of Aucon, two short earthquakes were felt which lasted ten seconds, and were noticed very perceptibly on board ship. The disturbance seemed to travel from south-west to north-east. The first sound heard was like that of steam let off under water, and escaping to the surface. This soon increased to a thundering, rolling noise, like the sound of river-ice breaking up. The damage done by this convulsion of nature was very insignificant on shore, and no casualties have been reported at sea.

— The German Seewarte has received a bottle which was set afloat by the German bark Suahali, Capt. Fröhling, on the 18th of November, 1883, in 0° 40′ north latitude, and 21° 40′ west longitude. On the 4th of February, 1885, four hundred and forty-four days later, this was picked up in the neighborhood of the life-saving station at Fort Laundersdale in the Straits of Florida, in about 26° 10′ north latitude, 80° 05′ west longitude. The end of the bottle's drift was about thirty-seven hundred and forty nautical miles distant from where it was set afloat, in a N. W. by W. ¾ W. direction. It is more probable that instead of taking that direction which leads through the Providence Channel, and then across the strongest part of the Gulf Stream, the bottle first drifted with the equatorial current west-north-west, then north from Trinidad into the Caribbean Sea, south of Jamaica, and by Cape San Antonio into the Gulf of Mexico, and finally with the Gulf Stream through the Straits of Florida to the place where it was found. According to this hypothesis, the drift was about forty-five hundred and fifty nautical miles for the four hundred and forty-four days, which makes an average daily distance of ten miles and a quarter. The Seewarte has also received a bottle which was thrown overboard from the German bark Suahali, on the 9th of December, 1884, in 44° 02′ north latitude, and 13° 16′ west longitude. It was found on April 8, 1885, at Buen in Ponte Vedra Bay, west coast of Spain, in about 42° 19′ north latitude, and 8° 45′ west longitude. So the bottle had probably drifted, in a hundred and twenty days, two hundred and twenty-three miles S. E. by E. ½ E.

— The crystalline form of quartz grains in some sandstones has been seen by many observers, while especial attention was called to these forms in the Wisconsin sandstones by Rev. John Murrish in 1870 and later. Mr. H. C. Sorby, in 1880, showed that such crystal forms were produced by the deposition of secondary quartz upon the irregular rounded surfaces of worn quartz grains. For the Wisconsin sandstones, the subject was taken up by Rev. A. Young, and later by Messrs. R. D. Irving and C. R. Van Hise, who have published an extended and valuable paper (*Bull. U. S. geol. surv.*, No. 8), with full illustrations, relating to the enlargement both of quartz and felspar grains; and for this the thanks of all micro-mineralogists and lithologists are due. Our authors conclude that their results prove that most, if not all, of the ancient quartzites, as well as many of the quartziferous schists, are composed in the main of fragments cemented together by a secondary siliceous cement.

— By the gift of the Hon. Elbert E. Fairman, LL.D., of Warsaw, N.Y., all that remains of the collection of birds made by the famous John J. Audubon is now in possession of Amherst college, Massachusetts. There are about six hundred skins of birds in the collection, some of which are labelled in the handwriting of Audubon himself, and many of which are the typical specimens by which the species were determined. As the collection has been stored in camphor chests for the last few years, and the skins were unmounted and old, many of them could not be advantageously mounted. About one hundred of them, however, have been handsomely put up by Prof. H. A. Ward of Rochester, and are now well exhibited in the Appleton cabinet of the college. Also there have been added to this collection by the same donor several of the rarer California birds, which have been discovered since the death of Mr. Audubon.

— The Paris students, according to *Nature*, are making extensive preparations for celebrating the one hundredth birthday of Chevreul, the veteran chemist, who has been a member of the Academy of sciences since 1826.

— A movement is on foot in Christiania, says *Nature*, at the instance of the Society for the promotion of the Norwegian fisheries, for the establishment in the Christiania fiord, near Dröbak, of a biological station for the hatching of sea-water food-fish and salmon, in consequence of the great success of other stations along the coast.

— The American ornithologists' union will hold its next meeting in New York on Tuesday, Nov. 17.

— The expedition which the Norwegian government despatches this summer to the coast of Finland is to ascertain whether there are banks or fishing-grounds far from the coast. Hitherto all fishing has been confined to the shore.